clusions under severe and prolonged tests, such as those attempted in Fritz Müller's "Für Darwin," and in Weismann's study of the markings of Lepidopteran larvæ. As Semper pointed out long ago (and too much insistence cannot be laid upon it), the present need is not for fine-spun theory—we have theories galore—but for the judicious compilation of facts selected where the leverage will tell, facts which shall either upset or confirm—it matters not which—the theory of natural selection.

The book before us must be classed among the speculative works just mentioned; and the gist of the author's views may be gathered from the following paragraph, written in 1880 (p. 401):—"Every permanent new form (species or variety) commences with the isolation of individual emigrants, separated for a prolonged period from the habitat of some parent species which is in the stage of variability. The active factors in the process are: (1) adaptation of the immigrant colonists to the external conditions of the new habitat (nutrition, climate, soil-composition, competition); and (2) the impression and development of the individual characteristics of the first colonists upon and in their posterity by reason of the breeding between near kin. This formative process ceases as soon as, owing to rapid multiplication, the levelling and compensating effects of intercrossing make themselves felt, resulting in and maintaining that uniformity which characterizes every good species and permanent variety." Wagner's hypothesis exalts the importance of geographical isolation at the expense of natural selection, and thus approximates, both at starting-point and conclusion, to Mr. Gulick's recent theory of "divergent evolution through cumulative segregation" (Journ. Linnean Soc., vol. xx. p. 189), though in detail the respective courses taken by the two writers are by no means identical.[1]

Consisting of a reprint of articles published between 1868-86, mainly in *Kosmos*, *Das Ausland*, and the *Allgemeine Zeitung*, the matter of the book has been long before the public, and its conclusions have been attacked from time to time by Haeckel, Weismann, Oscar Schmidt, and others; a translation of the first, and perhaps the most important article, has appeared in London (Stanford, 1873): criticism of the theory in this place is therefore unnecessary. The present reprint is edited by Wagner's nephew and namesake, in accordance with a wish expressed some time before his death in 1887, and contains, besides the articles previously published, a biographical sketch by Dr. von Scherzer, and editorial introductions; while the last 127 pages are devoted to an attempt of the editor to build certain recent discoveries, such as those of the *Challenger*, into the original structure. It is hardly necessary to say that, being a close-printed German octavo of 667 pages, the book possesses no index.

*Sylvan Folk.* By John Watson. (London: T. Fisher Unwin, 1889.)

MR. WATSON expresses much contempt for what he calls "the dry bones of science." We are not sure that we quite understand what he means by this expression, but it evidently does not imply that he dislikes results obtained by careful and exact observation. In the present little volume he gives ample proof that he often brings himself face to face with Nature, and that he knows how to interpret many of the innumerable signs and symbols which are readily misunderstood, or altogether overlooked, by less careful inquirers. Mr. Watson is especially happy in his notes upon the ways of birds; but he has also interesting chapters on mice, voles, and shrews, on red deer, fallow, and roe, on British seals, on British fur-bearers, and on "Nature by night." There is not much that is absolutely new in any of the information he has brought together; but his descriptions are so fresh—they suggest so vividly the idea of happy hours spent among attractive scenes in the open air—that they will give genuine pleasure to everyone who reads them. The book will be especially interesting to young readers, who will be glad to learn that it depends very much upon themselves, according to Mr. Watson, whether they shall be on terms of intimacy with the wildest woodland creatures. Mr. Watson thinks that the power of attracting wild creatures was once a much more common possession than it is now.

*A Practical Guide to the Climates and Weather of India, Ceylon, and Burmah, &c.* By Henry F. Blanford, F.R.S. Pp. 369. (London: Macmillan and Co., 1889.)

THE appearance of this book is very opportune. The Indian Meteorological Office has been in existence for some twenty years, and inasmuch as the region over which its operations extend comprises a very considerable area of the earth's surface, representing climatological conditions of the most varied character, a general *résumé* of the information as to these conditions is one of the most important contributions to climatology that could be made.

Mr. Blanford has well fulfilled his task. He says his work "is not addressed to meteorologists and physicists, . . . . but more particularly to agriculturalists, medical officers, engineers, pilots and other seafaring men, and to those others of the general public to whom the weather and the climates of India and of its seas are practical and not scientific objects of interest."

The book is divided into two parts: (1) the elements of climate and weather; (2) the climates and weather in relation to health and industry.

The former is naturally the more technical, while the latter appeals to the general public, as it gives a detailed description of the climates of the principal and most frequented hill stations, as well as of the plains, under which latter general head the different provinces or districts receive each a separate notice.

One section is specially devoted to the storms of the Indian Seas. In their discussion Mr. Blanford is a pronounced adherent of the spiral in-draft theory in contradistinction to the old circular theory and the well-known "eight-point rule."

About the most valuable chapter is the last, which is mainly occupied with rainfall and evaporation. The questions relating to these are of paramount importance for the bare existence of millions of the population. Such a famine as that of 1834 in the Doab was sufficient to induce the authorities of the day to construct the Ganges Canal, the greatest work of the kind in the world, and one which has in a great measure corrected the injurious effects of irregularity in the rainfall.

The appendixes give the tabular results of the instrumental records, which are required to substantiate the general statements contained in the previous pages.

The work is a most creditable production, and it will long remain the standard authority on any question bearing on the climate of the Indian Peninsula.

*The Unrivalled Atlas.* Enlarged Edition (18th). (London and Edinburgh: W. and A. K. Johnston, 1889.)

A NEW and enlarged edition of this atlas has just been published. The forty maps which it contains are well engraved and especially full of information concerning railway communication, whilst the fact that the index contains 20,000 names of places, with their latitude and longitude, testifies to its completeness. An extension of the atlas has been made by the addition of two classical maps, with an index to them, two physical maps of the British Isles and Europe, and two astronomical plates, each being accompanied with descriptive letterpress. A misleading paragraph occurs in the explanation of tidal action. We read: "The moon exerts a much greater influence on the production of tides than the sun; for, though its mass is excessively small in proportion, it is four hundred times nearer the earth." The inference that

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a beginner would draw from such a paragraph would be that the predominant effect of the moon in causing tides was simply due to its proximity to the earth, whereas the fact cannot be too strongly insisted upon, that it is the differential attraction of the sun and moon upon the earth's surface and centre that causes tidal action. The atlas is complete and trustworthy.

## LETTERS TO THE EDITOR.

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### Coral Reefs.

I BELIEVE all the questions asked by Captain W. Usborne Moore in NATURE of June 27 (p. 203) have been already answered in my papers on this subject. However, as I have examined the reefs at Kandavu, Matuku, Ovalau, and other places at the Fiji Islands, I may attempt to answer his questions with reference to these special cases.

According to Mr. Darwin's view, an island equal in extent to the lagoon of an atoll has sunk from view and the circular reef that has grown up marks approximately the position of the ancient coast line. In the case of a barrier reef, land equal in area to the lagoon channels has been submerged, the barrier reef here, too, marking the position of the ancient coast line. The opponents of Mr. Darwin's theory hold that there is no sufficient evidence that any such land once occupied the site of the lagoons and lagoon channels. I believe they might go further, and hold that, had such extensive submergence taken place, these lagoons and reefs would necessarily present features other than those by which they are everywhere characterized in our seas at the present time.

We hold that all the phenomena presented by these coral reefs and islands can be better explained on other principles without calling in subsidence; that slow subsidence or elevation or rest would only modify in a minor way the general features of a reef; that any one of the three kinds of reefs may be formed indifferently in a rising, sinking, or stationary area. In a sinking area the lagoons would probably be relatively deeper, the reefs narrower, and the islands on them small or absent; in a rising area the lagoons would be relatively shallower, the reefs broader, and the islands more numerous or united into a continuous band of land.

In surveying the coasts of any volcanic islands situated in the ocean where there are no coral reefs, Captain Moore must know that these are sometimes surrounded by banks extending much further seawards in one direction than in another. These banks are doubtless formed of the loose materials of the island, which are spread out by wave action. The position of these banks depends on the nature of the rocks in the different parts of the islands; in the case of Graham Island, the whole island was spread out, forming a submerged bank. In addition to the nature of the materials, the depth over the banks depends also on the extent and depth of the surrounding seas. The 20, 50, or 100 fathom line follows sometimes the shape of the coast within, sometimes differs widely from the shape of the coast, just the same as barrier reefs do.

The Fijis are, in my opinion, such a volcanic group, where the shallower waters have now become the home of myriads of lime-secreting organisms. These have built up the wholly submerged banks into atolls, and the banks around islands into barrier reefs, the depth and distance from land most favourable for vigorous coral growth being determined by a variety of local circumstances. When the reef reaches the surface, it spreads seawards. There is, of course, plenty of living coral in the lagoons, but this becomes less and less as the reef becomes more continuous and less oceanic water is admitted. The growth of corals is always, however, much less vigorous and much less rapid in lagoons than on the seaward faces of reefs. The position of the opening in the reef is determined by local conditions, such as the mud from rivers in the case of barrier reefs. I have elsewhere fully explained my view as to the removal of dead coral, and even coral heads and islands, from the lagoons, through solution of the lime by sea-water.

I cannot have seen as much of Kandavu as Captain Moore, but what I did see in no way led me to believe that it was a sinking island;[1] indeed it was here that, not being able to apply Mr. Darwin's theory in explanation of the phenomena of the Kandavu reefs, I commenced to doubt it altogether. The coast around Cape Washington (the western end) appears to be particularly unfavourable for the formation of extensive reefs, because of the high land (2750 feet) and deep water, hence we have only narrow fringing reefs or no reefs. At the north end the extensive banks surrounding the islets are, on the other hand, specially favourable for coral growth, owing to the almost complete absence of rivers and of detritus from the land; here we have what might be called an atoll, except for the presence of the small rocks, which, after reaching the surface, has extended seawards.

If Captain Moore thinks the rock, Solo, was once an island about four miles in diameter and of considerable height, he must explain how in sinking it has left a lagoon around the remaining rock with an average depth of not more that 12 fathoms. He must also explain the nature of the bank extending west from Ono, where there is no continuous barrier.

It is in every way desirable that practical surveyors like Captain Moore should take an interest in these theoretical views, and their observations will be none the less welcome and appreciated whatever side they may take in the controversy. The more observations accumulate the more does it seem to me probable that there never was a barrier reef or atoll formed after the manner required by Mr. Darwin's theory. If Profs. Dana, Bonney, Huxley, and Judd, would mention any one barrier reef and any one atoll that they believe undoubtedly to have been formed in accordance with the subsidence theory, then I think it might be possible within the next few years to undertake a thorough examination of these with the view of testing the rival theories.

*Challenger* Office, Edinburgh. JOHN MURRAY.

I HAVE been immersed in examinations, and away from London for a few days, so that I did not see Mr. Guppy's letter in NATURE of June 20 (p. 173) till it was too late to reply. I will now only ask space for a few last words. To me the matter does not appear to have "resolved itself into an affair of outposts." The position which I was led to take up from the study of the recent literature on coral reefs, and which I had hoped that I had made clear in my last letter, is this—that till Mr. Guppy can produce cases of *growing reefs* at depths well exceeding 25 fathoms, isolated instances of the occurrence, at such depths, of living corals which are among the reef-builders do not really help him; and that till he can do this he is only supporting hypothesis by hypothesis. For example, I have not seldom, in the Alps, gathered phanerogamous plants, flourishing and in full bloom, at elevations of eleven or even twelve thousand feet above the sea; but I should not direct anyone to this mountain zone who desired to pick a posy of Alpine flowers.

Into the remainder of Mr. Guppy's letter it would be waste of time to enter, or to continue this controversy, for two reasons: one, that evidently Mr. Guppy and myself differ widely as to the nature of hypothesis, and the difference between assumption and proof; the other, that I trust, before another number of NATURE appears, to be among the Alps (with strict orders against forwarding letters or papers), so that any return shots will fail to reach me, until, in this rapidly-moving world, they have passed into the region of ancient history.

June 28. T. G. BONNEY.

BEFORE replying to Captain Moore, I should bear witness to the searching character of his questions, which quite makes up for the fact that during his valuable services of many years abroad he has been, fortunately perhaps, beyond the reach of recent coral reef literature.

However, I would first point out that when he speaks of the second edition of Mr. Darwin's "Coral Reefs," which was published fifteen years ago (1874), as given to the world "after forty years of deep research into various problems of Nature," he surely forgets that after the first edition of 1842 the author had no further acquaintance with coral reefs. Captain Moore also omits to reflect that during the interval between the two editions Dana and Jukes were the only two principal observers that stood up

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[1] See "Narrative of the *Challenger* Expedition," vol. i. p. 507.

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